

*Comments on
"Scientific Forecasts
in International Relations"*

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The initial disclaimers at the outset of the article that the authors seek neither to forecast nor to make a contribution to the "corpus of knowledge" in this area leave one with a sense of uncertainty about the context in which the issues raised must be viewed. It would constitute the essence of unfair practice to regard the article in any context other than that intended by the authors. The following observations are, therefore, of a more general nature by way of clarifying some of the issues raised in that article. These comments are presented in the order in which they are discussed by the authors.

(1) It is clear that little attention is devoted to forecasting in international relations. This is due not only, or even largely, to the absence of data—we have more information in variables such as energy projection, and consumption, than we know what to do with—but, more fundamentally, to the absence of underlying *theory* to guide a forecast (or prediction or projection) or to develop an internally consistent and useful simulation. It is the absence of theory, rather than data, that should be lamented by the authors in that (and most other) respects.

(2) Clearly, there is no consensus regarding the nature of forecasting. The authors refer to forecasting and prediction, but neglect projections and simulations. A critique of the issues that are broader than "semantics" in their nature would be more

complete were the distinctions between *projections* and *simulations* also included.

(3) What one *needs* to know about the future depends on what one's *purposes* are. Thus, it is not surprising that scholars and policy makers cannot agree on "what it is that they need to know." The large literature on belief systems, or ideology in foreign policy operational codes, can certainly shed light on the lack of consensus in this general area. The distinction between prediction, forecasting, projection, or simulation is useful to the policy maker largely in terms of his purposes and the degree of uncertainty with which he can be comfortable. That a group of scholars could agree upon a set of definitions will not make that agreement necessarily useful to the policy maker. It is, however, more important for scholars to agree upon *how to evaluate* the results of statements about the future.

(4) The *time horizon* of an exercise generating a statement about future outcomes depends on the purpose of the analyst and, accordingly, upon the theory employed (however implicit or ambiguous it may be) and upon data. Temporal distinctions are, unfortunately, the ones most commonly resorted to, not only in everyday life but, more to the point, in the policy makers' frame of imperatives.

(5) Upon reading the authors' statement on the issue of time horizon, one finds their clarification rather confusing. My statement (1974: 71-75) on the relation of methods appropriate to time horizon is based on empirical work and the writing of forecasting and simulation models. It is not an a priori approximation of the role of method to time horizon but, given my purposes and the substantive issues examined (namely, aspects of international conflict), I have found that the rough correspondence in Figure 1 (1974: 74) is useful. While one is more than prepared to be shown to be wrong, one can best be convinced only on the basis of results, actual modeling efforts, retrospective forecasts (predictions, projections, simulations), and so forth. In the absence of evidence, it is useful to retain a sense of skepticism regarding alternative assertions about this and other issues.

(6) The authors' observations on "crude prophecy" sounds,

suspiciously, like a defense of the practice. Therefore, one must plead a misunderstanding of their point.

(7) Agreeing upon *criteria* for evaluating statements about the future is critical. I am surprised and dismayed at the examples used. Leaving aside the weather forecasting (of which I know little), I am startled at the examples bearing on the role of technological innovations in population control. No population expert has, to my knowledge, attributed to birth control pills the responsibility for faulty population projects in the 1950s. (The term used by population experts is "projection" rather than "forecast.") A variety of other things were critical, including the lack of understanding of the interconnection of economic development with human reproduction. By the same token, it is unclear whether the development of male contraceptives will affect population change extensively. It is still a matter of controversy whether the simple availability of contraceptives is a determining factor in influencing fertility patterns. The theory of demographic transition—with its ambiguities and inconsistencies—still remains the most insightful statement on the issue.

(8) I am reluctant to consider weather forecasting a useful model for social science or international relations analysis. In graduate school, we were taught that one cannot generalize from mice to rats. With the wisdom of the years, I am now skeptical about the possibility of generalizing from clouds to countries.

(9) Somewhere, there is a misquotation. I must protest the authors' attribution to me the implication that I believe "all crucial innovations in international relations have occurred" (1974: 74). The issue posed in that context pertains to the complicated problems of making statements about future outcomes when breaks and discontinuities in the relationships in question are known (or even hypothesized) to exist.

(10) In defense of Professor Rosecrance's insightful work (1963), I still maintain that he has highlighted many problems involved in understanding and explaining system transformation. But never can one argue that methodologically his was the correct (or even the most useful) way of proceeding with fore-

casting international system change. (Nothing I have written, or modeled, bears any resemblance to Professor Rosecrance's approach, yet I must continue to stress the pathbreaking nature of his work.)

These ten observations are in the nature of minor comments. Let me conclude with several observations of a more serious nature.

I would like to be persuaded that my seven concluding observations (1974: 79-80) are "misplaced" and that certain blinders are not only appropriate but essential if we are to develop sound scientific forecasts that constitute an alternative to "crude prophecy." All seven "recommendations" must be taken into account rather than selecting one, out of context, with the implication that it illustrates how the others are also "misplaced." Also, one's defense of Professor Rosecrance's study (1963) cannot be used in lieu of one's own work to refute any arguments regarding the importance of *modeling endogenous system change*.

It is that body of work which has been done during the past several years that should also be subjected to criticism, commentary, or respecification. It effectively constitutes the *result* of the orientation expressed in the 1974 paper. With apologies for the cliché, the evaluation of the forecasts (or systematically generated statements about future outcomes) must be done with respect to the forecasts themselves, not solely in terms of the scholar's directives *about* forecasting approaches.

Nonetheless, it is indeed a source of encouragement that scholars of international relations are systematically looking into issues pertaining to contemporary orientations toward forecasting, simulation, projects, and predictions. Thus, the article is an addition to a large body of literature *about* international relations forecasting. However, one would be more encouraged if scholars were also to devote attention to the *evaluation* and assessment of existing work that has generated *actual* forecasts, some of which can be compared with the actual, known, historical or contemporary record, or with alternative statements about the future. Given the growing experience with generating actual (systematic) forecasts (projections or simulations) based

on theory and empirical data, it still has to be demonstrated—more rigorously—that the concluding observations or directives written in the 1974 article can, to date, be regarded as “misplaced.”

